

continued use of pilocarpine might have inhibited pupillary dilatation and clear photography, and this of course did sometimes happen. In some cases the problem was overcome by cessation of pilocarpine for a longer period before a subsequent attempt at photography, and I gained no impression of a grossly different photographic success rate in patients on miotics. On the other hand it is possible to argue that patients on nonmiotic therapy are more likely to be those who have lens opacities, and consequently they will be the ones less likely to give clear photographs. Some doubt must remain on this sort of problem, but whether such uncertainties can be completely avoided is another matter; for example, glaucoma patients with more than a certain degree of cataract will always be excluded from this sort of study, so some selection is inevitable. Mr Sutton is quite right in saying that such factors as the interval between photographs and the number of visits were not standardised. This was the point of my comment on p.455 that 'photography at regular intervals' should be aimed for in any future study.

With regard to the observed overall incidence of disc haemorrhages in glaucoma, it is to be expected that this will rise as the number of observations increases. Unfortunately, as was pointed out, the data were insufficient to allow accurate extrapolation to determine the incidence which could be expected with a very large number of observations on each patient. Nevertheless, consideration of the results of Table 1, for example by inspection of a simple graph, suggest that an overall incidence of around 30% is a reasonable minimum. It could easily be more. In this connection it is interesting that, in the study by Bengtsson *et al.*¹ to which Mr Sutton makes reference, somewhat similar results were presented as a graph of frequency versus number of observations, showing that 2/3 of glaucoma patients presented a disc haemorrhage within 4 visits, and it was stated that the hypothesis of disc haemorrhages occurring in all cases could not be rejected, although it was not proved. This effect of frequency of observation upon apparent incidence clearly has to be borne in mind when comparing groups of patients, and an attempt was made to do this. In Table 3, which related the incidence of haemorrhages to the cup:disc ratio, the average number of times eyes were photographed at various sizes of cup:disc ratio was given. This was also so for the relationship with level of intraocular pressure (Table 5). Mr Sutton says that I 'did not study the IOPs recorded at the time photography was done.' I did not really think that there was much point in doing this because of the varying preparation of patients for photography. Those on miotic therapy usually had their drops stopped, some for longer than others, those on nonmiotic therapy continued their treatment, some had acetazolamide for a day or two before photography, these variations being partly dependent upon previous response of the IOP to temporary cessation of treatment. In any case the level of IOP at photography probably bore no relationship to that at the time the haemorrhage occurred. The maximum IOP which had been recorded for each patient was chosen as the one by which cases could be divided into groups, because this method resembled the traditional division into 'low tension glaucoma' and 'chronic simple glaucoma,' but avoided the arbitrary choice of a dividing pressure level.

It does seem that the occurrence of disc haemorrhages in chronic simple glaucoma is worthy of more study. In particular one would like to know if haemorrhages really do occur at some time or another in all patients, whether they are part of the process leading to glaucomatous disc damage or merely one of the results thereof, and just what prognostic importance they have and so on. I hope that my paper indicated some of the advantages and disadvantages of basing investigations on photography, and I believe that it gave an idea of the scale of the investigation needed to get more accurate information than that which is available to us so far.

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Reference

- 1 Bengtsson B, Holmin C, Krakau CET. Disc haemorrhage and glaucoma. *Acta Ophthalmol (Kbh)* 1981; **59**: 1-14.

Herpes zoster ophthalmicus

SIR, In a recent article by Dr S. Lightman *et al.*¹ it was stated that as a result of a retrospective analysis of 1000 cases of herpes zoster ophthalmicus the great majority of the patients were 'healthy and therefore do not have diminished immunity.' The authors base these claims on the strength of a full blood count, differential white cell count and film, liver function tests, and electrophoresis, blood sugar, and chest x-ray. No attempt was made to assess quantitative immunoglobulin levels, and T cell function was not assessed.

As the paper is entitled a 'Medical review' of herpes zoster ophthalmicus, why was an account of family history of compromised immunity or the patient's past immunisation history not documented? A physical examination was not mentioned.

This paper highlights the danger of reaching conclusions based on retrospective analysis.

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- 1 Lightman S, Marsh RJ, Powell D. Herpes zoster ophthalmicus: a medical review. *Br J Ophthalmol* 1981; **65**: 539-41.

SIR, Active viral infection is associated with an alteration of T cell function, and assessment of T cell function in our patients on presentation with active herpes zoster would give no indication of the patient's basal immune status. The follow-up of our patients was for an average of 2 years, during which time no further infective episodes occurred, and therefore there was no medical indication for expensive immunological investigations. Family histories were taken from all patients and revealed no evidence of compromised immunity.

Since the most common age range of our patients was 50-70 years, immunisation histories are likely to be inaccurate, and most patients had difficulty recalling whether or not they had had chicken-pox.

These problems would be no different in a prospective study and do not detract from the conclusions reached in our paper.

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examples like this where clinical and descriptive terminological improvement is possible.

However, as the author indicates in the preface, the evolution of CT scanning has provided 'a wonderment beyond imagination' in orbital diagnosis. This wonderment is appropriate because only 8 of the 600 pages in this book are concerned with scanning. CT scanning is probably the most important diagnostic mode in orbital disease, and the absence of good examples of CT scans in the more common lesions is a major drawback to a book appearing in 1980.

This book is enjoyable to read and contains a great deal of information, but sadly a third edition encompassing the full impact of CT scanning will be necessary for the ophthalmologist of the 1980s.

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Book reviews

Orbital Tumors. 2nd edn. By JOHN W. HENDERSON. Pp. 607. DM.194.00. Georg Thieme Verlag: Stuttgart. 1980.

The ophthalmologist is usually the first person to see patients with orbital disease, and his clinical horizon is thus opened to a wide variety of systemic, neurological, and developmental abnormalities which do not occur within the globe. The second edition of this work on orbital tumours appearing 8 years after the first edition is therefore to be welcomed. The distinguished author has collaborated with a pathologist, G. M. Farrow, also from the Mayo Clinic.

The strength of this book lies in the fact that the author has personally studied a series of 764 cases of orbital tumour and observed their natural history over many decades. This is an essentially personal book, which is embellished with good clinical photographs, supported by x-rays and numerous examples of pathological specimens. Presentation is excellent, and the bibliography is historically extensive, but many recent references of relevance seem to be omitted.

The material is considered in depth, with each condition receiving historical analysis, followed by aspects of clinical diagnosis and details of the pathological features. In any book covering so extensive a field there are omissions, but the failure to emphasise opticociliary shunt vessels as a characteristic feature of the nerve sheath meningioma is disappointing. Instead the authors describe papilloedema (presumably they mean disc oedema) and striae of the posterior wall of the eye (differential features of retinal and choroidal striae are now well established). There are many

Az Endogen Uveitisek És Általános Orvosi Vonatkozasaik. By LUGOSSY GYULA. Pp. 407. 116 Ft. Akadémiai Kiadó: Budapest, Hungary. 1980.

Dr Lugossy is well known to English ophthalmologists from his contributions to the meetings of the Ophthalmological Society of the United Kingdom. This monograph is the result of a lifelong study. The material consists of patients with uveitis observed and followed over 30 years in the Ophthalmic Department of the Rheumatology Institute in Budapest. His results are complemented by replies from questionnaires from hospitals in Hungary and other centres interested in the problem.

Chapters are devoted to epidemiology, classification, experimental uveitis, role of auto-antibodies, clinical and paraclinical investigations, and immunology. Two final chapters discuss the pathology of uveitis and the whole spectrum of aetiology. References at the end of each section are up to date and taken from English, French, German, and Russian literature.

Lugossy's writing is lucid and the presentation good. It is a pity that the print is on such glossy paper. For those who read Hungarian the book is inexpensive. At today's exchange rate it costs less than the price of a throw-away paperback and is strongly recommended to clinicians.

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